ANALYSIS

Edited by Bernard Mayo, with the advice
of A. J. Ayer, R. B. Braithwaite, Herbert
Dingle, A. E. Duncan-Jones, P. T. Geach,
C. A. Mace, A. M. MacIver, and H. H. Price

Volume 19
1958–1959

BASIL BLACKWELL · BROAD STREET · OXFORD

CONTENTS

ol. 19 No. 1 (New Series No. 67) October 1958

ANALYSIS

CONTENTS

THREE SHILLINGS AND SIXPENCE NET

BASIL BLACKWELL · BROAD STREET · OXFORD

ANALYSIS COMPETITION. PROBLEM No. 14.

Problem No. 14 is set by Professor D. J. O'Connor, of Exeter, and is as follows:

If I carefully examine a visual after-image, what am I looking at and where is it?

Entries (of not more than 600 words, typewritten) should be sent *to the Editor of* ANALYSIS by **Friday, December 19th, 1958**. A report, with any winning entries, will be published in ANALYSIS for March 1959.

REPORT ON *ANALYSIS* PROBLEM No. 13

Is 'Paradise Lost' a general name, proper name, or what?

By P. H. NOWELL-SMITH

THIS problem produced a poor crop of entries in quality and quantity. Perhaps because it was No. 13, perhaps because it was difficult to answer in more than six words or less than six thousand. Several competitors opened with the impeccable short answer "It's a title". But where do we go from there, unless to embark on a book? Peter Swiggart, who though he writes from a Department of English, is not quite uncontaminated by philosophy, ended with the moral I wanted drawn. His entry is printed below because, in spite of many obscurities and some errors, it is the only one that does not lean too heavily on some familiar philosophical incantation.

Of these the worst is 'type/token'. I soon became tired of being told that 'P.L.' is the proper name of a poem, which is a type of which particular copies, readings and even experiences are tokens. This simply will not do. One example will serve for many. "As a matter of fact *Paradise Lost* happens to be a general thing, of a kind sometimes called a *type*, along with *e.g.* the Union Jack, the game of chess and Beethoven's Fifth Symphony." This gives a breadth to 'type/token' which destroys what candle-power it may once have had. Paradise Lost will go a long way with the Fifth Symphony; but it parts company with the Union Jack and the game of chess very early, at the point at which we see that there can be many Union Jacks and many games of chess but not, in the same way, many Paradise Losts. (A minor point against Mr. Swiggart here. I suspect that it is

euphony that prevents us saying ' Paradise Losts '; for I feel less qualms about saying of a collector that he has several Iliads. Also we can use the indefinite article as in ' Dryden was a good poet, but he never wrote a Paradise Lost '. Nevertheless this use is certainly queer—perhaps a contraction for 'poem comparable with P.L.',—whereas 'a game of chess' is in no way queer.)

Most people plumped for the proper name view, some adding quite rightly that since objects of many different sorts have proper names we should not expect all proper names to be alike in all logical features. Poems are not people. But there was one argument commonly used to support this view which misses the point badly. Harris Plitter put it most clearly; so he shall be the victim. " Multiplicity of reference is not sufficient to destroy the proper-name-like status of ' P.L.'. . . . There are lots of people with the proper name John Smith." The premise is true enough; but it won't prove the desired conclusion, for there is a confusion as to what the multiplicity is a multiplicity *of*. If there is only one poem entitled ' P.L.' we have a proper name (of that poem) without multiplicity of reference. If there are many, the analogy with ' John Smith ' holds; for we have a proper name (of a poem) which may be used to designate any of a number of poems. But Mr. Plitter's multiplicity consists of " tokens, exemplifications, concrete instantiations " (of Milton's poem); and now the analogy breaks down. For these are all copies, readings, perusals, interpretations etc. *of Paradise Lost* (the poem), whereas John Smiths are not examples or concrete instantiations *of* John Smith (the man). Secondly, the multiplicity is carried by the general nouns, ' copy ', ' interpretation ' etc., not by ' P.L.'.

Mr. Plitter, in the same vein, raps Webster over the knuckles for saying that a proper name " pertains to or designates one individual only ". This dictum is slovenly because it invites the interpretation that a proper name cannot be used to designate more than one individual, which the case of ' John Smith ' refutes. But let us be charitable to Webster; perhaps he meant " one individual only at one time ". I beg leave to enlarge on this. It is instructive to compare the entries in the O.E.D. under ' proper ' and ' common '. Under ' proper ' we have " used to designate a particular individual object; opposed to common", and under ' common ' we have " applicable to each individual of a class or group ". This is surely right. Proper names do designate particular individuals, and ' John Smith ', even though applicable to many individuals, is *not* " applicable to each individual of a class or group ". For this must be taken to mean

" applicable to each individual *as* a member of a class or group ". Moreover John Smiths do not form a class or group. If we decided to impose a special tax on all bearers of this name, they would. But can we not arbitrarily form the class of all bearers of the name ' John Smith '? Certainly we could, and in that case we should want to invent a general name applicable to each individual of that class. We might, very likely, choose ' John Smith ' to be this general name.

But it is not my job to dilate on proper names. Mr. Plitter deserves credit for putting clearly the two main doubts that arise from the short answer " It's a title, and therefore a proper name of a sort ". (i) If it is a proper name, it is the proper name of a poem and this may " produce symptoms reminiscent of Quine's eidophobia ". (ii) " One might be bothered by the fact that there are lots of copies ". But, alas, he looks the first doubt boldly in the face and passes on—to the second, which he treats inadequately.

Surprisingly few competitors made the point that ' P.L.' might (like ' Shylock ' or ' Daniel ') be used now as a proper name, now as a general name; indeed the obvious possibility of status varying with context was not sufficiently exploited. (Kenneth Stern pointed out that ' the Editor of ANALYSIS ' is a title only when it is used to address him; otherwise it is the name of someone's job. But he did this only in a footnote.) Donald Henze began in fine style by listing no less than twelve different uses of ' P.L.', which he grouped under the headings ' proper name ', ' general name ' and ' syncategorematic ', the last-named being uses which do not presuppose the existence of anything called ' P.L.'. He also ended with a moral which deserves to be quoted. " My examples are not exhaustive of the uses to which the name of a poem might be put, but I have given a few in order to show (i) that what sort of name (if any) a term is, is a function of that term's use in context, and (ii) that an analysis of a term's use can be more instructive than a decision as to what sort of name it is, because *one* sort of name may have *many* uses." Unfortunately his passage from the examples to the moral seemed to me quite wrong. For example, the phrase ' my initial exposure to P.L.' does, as he says, refer to a particular; but the particular is not P.L. and the particular reference is not carried by the name ' P.L.', which it would have to be to support his argument.

All in all, I am no clearer about types and tokens than I was before, which I find disappointing.

IS 'PARADISE LOST' A GENERAL NAME, PROPER NAME, OR WHAT?

By PETER SWIGGART

TWO students and two professors are engaged in separate conversations. One student asks the other: "I want to read *Paradise Lost*. Do you have a copy?" A similar question is asked by a professor: "I am going to lecture on *Paradise Lost* next semester. Which text would you recommend?" There is a superficial ambiguity in both cases. The student wishes to read both *Paradise Lost* and a copy of *Paradise Lost*. The professor expects to lecture both on the poem and on one of its texts. Both student and professor are no doubt clear about their specific intentions. But they would be hard-pressed to state what the name 'Paradise Lost' really names. The student might argue that any poem is a unique but repeatable sequence of word-symbols. The professor could add that radically different versions may and often do exist of the "same" poem by the same author. How can a poem be the same poem if its text has been completely altered?

It is important to realize that this ambiguity is not grammatical, since 'Paradise Lost' is invariably used as a proper name. We capitalize both words, and refrain from saying 'Paradise Losts'. In speaking of a copy or copies we usually add 'of *Paradise Lost*' as if the printed words did pertain to a unique if elusive reality. In this grammatical sense 'Paradise Lost' is undoubtedly a proper name and no ambiguity or puzzle can be said to exist.

A puzzle is created only if we take the above ambiguity seriously and try to examine the term according to its symbolic "meaning" or its relation to an underlying reality that may or may not be unique. From this point of view the question can be restated as follows: "Is 'Paradise Lost' the name of a unique object, a general category of objects, or what?" This new question brings to mind a large body of art objects that cannot be definitely classified as being either unique or general. In the case of poems, novels, film showings, bronze castings, or wood block prints the impossibility of establishing an unambiguous referent for each title name is evident. A similar point can be made concerning paintings, statues, and other art works generally considered to be unique. One can say with equal validity that one is looking at a reproduction of Picasso's 'Guernica' or,

speaking of the same experience, that one is looking at 'Guernica'. The puzzle concerning the referent of 'Paradise Lost' cannot be dissolved as long as one insists that 'Paradise Lost' must be the name either of one or of many things—that it must bear some clear and logical relationship to the physical world. Proper handling of such terms is incompatible with the orthodox metaphysical structure, the view that words are invariably symbols for things, that underlies the pronouncements of most modern logicians. To a certain extent their view is implied, certainly facilitated, by the Indo-European grammatical systems. In using 'Paradise Lost' ordinarily we must decide arbitrarily whether to treat it as a general or as a proper name. But the need to classify grammatically does not force upon us the conviction that 'Paradise Lost' must have a clear referent, physical or otherwise.

The revised question, and the type of answer it seems to call for, point to a kind of layer-cake view of language. Below the top layer of ordinary usage may be discovered ideal logical forms that are really "meant" by this word or that statement. Below these logical forms is said to exist a non-verbal level of reality, the world of discernible objects to which the linguistic forms, both ideal and ordinary, refer. Wittgenstein has criticized the tendency of logicians to confuse the first two levels (see *Philosophical Investigations*, 94–107). To my mind it is even more important to question the over-simple view of word-thing relationships which is responsible for our rigid conceptions of general, proper, and what-have-you names. Instead of trying to classify such terms as 'Paradise Lost' into word-thing categories, logicians should re-examine their categories, or their metaphysics, in the light of such terms.

University of Texas.

A late entry, which was not seen by Professor Nowell-Smith until after he had written his report, is now judged by him to be "much the best", and is accordingly printed below.

'PARADISE LOST'—GENERAL NAME, PROPER NAME, OR WHAT?

By Ivor Hunt

SOMETIMES poets think up titles for their works. Sometimes these titles suggest themselves, as with our example, sometimes not. Sometimes no such title seems appropriate, or necessary, and the poem receives as a (perfectly satisfactory) title its own first line placed in quotation marks. The title of an epigram might be simply the quotation of the epigram.

Now are titles proper names? We require of a proper name that it be conventionally (we prefer, appropriately) assigned to an individual regarded as unique (i.e. as *that* individual). Now we might hold that titles satisfy this requirement, but then a puzzle will arise. For what of 'Hiawatha'? If the name of the poem is 'Hiawatha', this name is ambiguous, for it is also the name of a brave. But if the name of the poem is "'Hiawatha'", again this name is ambiguous. For is the statement "There are only two trochees in 'Hiawatha'" true or false? This puzzle shows, I think, that there is an oddity in the way quotation marks behave in titles—reflecting, perhaps, a peculiarity in the things named by them (properly, it is still open to hold).

For it seems still more unsatisfactory to hold that e.g. 'Paradise Lost' is a general name. For then there should be a class of things to which it applies. But the only candidates for membership of this class are *copies* of 'PL'. But "'PL'" is not related to my copy of 'PL' in at all the same way that 'hat', 'silk' or 'glossiness' are related to my glossy silk hat. My copy *is* the poem 'PL' in a way in which it would be nonsense to speak of my hat's being any of these things.

On the other hand, there is a generality in the notion of 'PL' which might be brought out by saying "Whenever you have *these* words in *this* order . . . you have (a copy of) the poem 'PL'. (But, for the difference, compare: "Whenever you have lines in *this* arrangement you have a rhombus.") This makes your MS an instance of the general name 'copy of 'PL'', but it doesn't make it an instance of 'PL'. 'PL' is on every cultivated person's shelf, but this means: on every cultivated person's shelf you will find a copy of 'PL'. From this it does not follow that that of which there are copies is on anyone's shelf.

"That of which there are copies"—where is it? Not in the British Museum, for Milton might have dictated his poem direct

to the compositor (though, no doubt, this is the origin of the expression). Laid up in Heaven? Where does the Type of all tokens dwell? The unease we generate by adhering too rigidly to *one* model for generality can be seen in such ways, that we might be driven to, of putting the point as: "What makes a MS 'PL' is *uniquely* the same in all instances", or, more tersely: "'PL' can be instantiated only by itself".

It might be objected that this whole discussion treats the poem as though it were merely marks on paper. Absurdly. For suppose, instead of 'PL', we used for a title, as I suggested earlier, the poem itself between quotation marks. Then this would be indeed the (general) name of a class of sets of marks on paper. But in so far as it was the name of *the poem*—well, the poem would have named itself!

Christ Church, Oxford.

SYMMETRY, TRANSITIVITY AND REFLEXIVITY

By DAYA

IT is commonly assumed that relations which are both symmetrical and transitive are also necessarily reflexive in character. Russell, for example, writes: "It is obvious that a relation which is symmetrical and transitive must be reflexive throughout its domain."[1] The obviousness of the necessity may perhaps be not so "obvious" after all. At least, Russell has not here given any reason to make us accept the "obviousness" except one which we shall discuss later on.

A supposedly clinching logical proof can, however, be given for this "obviousness". Given symmetry, if xRy then yRx: and given transitivity, if xRy and yRx then xRx which is the symbolic characterisation of reflexivity itself. Thus, reflexivity is seen necessarily to follow from symmetry and transitivity taken together.

The proof seems logically unexceptionable, but if we consider certain concrete examples, we are seemingly led into difficulties. Take, for example, the proposition: *John is different from Tom* and *Tom is different from Harry*. It seems clear that if John is different from Tom, Tom must also be different from John and if John is different from Tom and Tom is different from

[1] *Introduction to Mathematical Philosophy*, p. 16.

Harry, John must also be different from Harry. The two characteristics of symmetry and transitivity are, thus, satisfied in this case. But obviously the relation does not thereby become reflexive, for it certainly is not true that John is different from John.

It may perhaps seem difficult to dispute that, at least in this particular instance, symmetry and transitivity do not lead to reflexivity. Yet, the formal proof that it must be so, seemed completely valid. Where, then, is the clue to this seeming incompatibility between the concrete instance and the logical proof?

Logicians have sought to solve the difficulty by denying the transitivity of the relation of " difference ". It is contended that if x is different from y and y is different from z, then x need not be different from z, for x and z may be identical. Russell, for example, in denying the transitivity of the relation " greater or less " among numbers gives the reason that " if x is greater or less than y, and y is greater or less than z, it may happen that x and z are the same number ".[1] Similarly, he has sought to deny the transitivity of the relation " brother or sister of ", which ordinarily would be regarded as both symmetrical and transitive and yet irreflexive, on the ground that " if x is brother or sister of y, and y of z, x may not be brother or sister of z, *since x and z may be the same person* ".[2]

This is the only reason, to my knowledge, that has ever been given by Russell or anybody else for denying the transitivity of these relations which, though symmetrical, are yet irreflexive in character.[3] It should be noted that the symmetrical character

[1] *Ibid.*, p. 33. [2] *Ibid.*, p. 31. Italics mine.

[3] It may be questioned by some whether the *only* reason for denying the transitivity of such relations as that of " difference " consists in the possible *identity* of x and z as suggested by Russell. x and z may not be identical and yet if x is different from y and y is different from z, x may not be different from z, for x and z may be *similar*. It is the possible *similarity* and not the possible *identity* of x and z that makes the relation intransitive. Thus, unless we choose to treat bare numerical difference as " being different ", we cannot find the *sole* reason for denying the transitivity of such a relation in the Russellian argument.

Prima facie, the objection seems pretty strong. But, if closely examined, it would appear to thrive on an ambiguity. " Similarity " is a tricky word. If two things are " similar ", that does not mean they are not " different ", but *only* that the points of difference are not considered as relevant or important by us. With a slight change of context, interest or purpose, the " differences " may loom larger and become more important than the " similarities ". Two mango-trees x and z, for example, are similar and each different from the banyan-tree y. But this does not mean that the trees x and z cannot be different in, for example, the taste of their fruits or the girth of their trunks or the width of their leaves or the abundance of their harvest or the time of their ripening etc. etc. We can, of course, think of examples where x and z have complete *qualitative identity* with just numerical diversity. Two *equal* specimens of an element with identical physical and chemical properties may be said to exemplify such a situation. Here x and z could not be said to be different

Footnote continued on page 9

of these relations has never been questioned and that the argument for denying their transitivity *has nothing to do* with the symmetrical, asymmetrical or non-symmetrical character of these relations. The argument, it has not been noticed, if valid, proves too much. It rules out the transitivity of all relations which are irreflexive, independent of the question whether they are symmetrical or asymmetrical in their nature. For example, if x precedes y, and y precedes z, x may not precede z, since x and z may be the same thing. This appears so obviously correct that it seems amazing to find Russell writing on the same page "if x precedes y and y precedes z, x *must* precede z".[1] Why must x precede z if z may refer to the same thing as x and x cannot precede itself? Russell writes further: "This may be illustrated by the same instances as before: *less*, *earlier*, *left of*".[2] Here "less" is given as an example of a transitive relation, while just two pages after, on page 33, the relation "greater or less" is considered as not transitive, "for if x is greater or less than y, and y is greater or less than z, it may happen that x and z are the same number".

Prima facie, no relevant difference seems to be made to the argument by the substitution of "greater or less" for "less" to make the one intransitive and the other transitive.[3] The argument for the intransitivity of the relation "less than" can almost be put in the same words: "for if x is less than y, and y is less than z, it may happen that x and z are the same number". Russell would, of course, deny the very *possibility* of putting the argument in such a way. It cannot even possibly happen that x and z are the same number, for such a possibility is ex-

Footnote 3 continued from previous page
from each other in any respect, even though they may each be different from another element y. But this is only because we have assumed *complete qualitative and quantitative identity* between them and *decided to ignore* the only difference between them, i.e. numerical diversity. This, obviously, comes to the same thing as the Russellian argument from the identity of x and z. Thus, either "similarity" between x and z does not exclude "difference" between them or it reduces itself to the Russellian identity with the postulation of complete identity and the decision to ignore numerical diversity.

[1] Ibid., p. 31. Italics mine. [2] Ibid., p. 31. Italics author's.

[3] The relation "greater or less" may be interpreted in a manner different from the one in which Russell has understood it here and in that case the usual Russellian argument for the denial of transitivity would not be the sole ground for its denial here. If, for example, "greater or less" is understood as, say, "taller or shorter" then obviously the reason for the denial of its transitivity would not lie in the possible identity of x and z. A tree may be "taller or shorter" than a building and a building "taller or shorter" than a telegraph-pole without it necessarily following that the tree must be "taller or shorter" than the telegraph-pole, since the two may be of the same height.

Such an interpretation, however, would remove it from our field of discussion altogether. We are interested *only* in those relations the *sole* ground of whose intransitivity lies in the possible identity of x and z, for we are certainly not denying that there are such things as intransitive relations.

cluded by the asymmetrical character of the relation " less than ". If x *is* less than y, and y *is* less than z, z cannot be the same as x, for if x is less than y, y cannot be less than x. The asymmetrical character of the relation does not permit the identification of x and z on which the argument was supposed to rest. In the usual Russellian argument, it is the symmetrical character of the relation that permits the identification of x and z, though he has nowhere explicitly stated it to be so. It is only on this supposition that some sense can be made out of his denial of transitivity to "greater or less" and his accepting it in the case of the relation " less than".

Unfortunately for the argument, it is not so much grounded in symmetry as in the possible identity of the referents of different symbols, an identity that does not make the relation between them invalid. Symmetry itself, for example, can be questioned on this ground. If x is " greater or less than " y then y may not be " greater or less than " x, for y may refer to the same number as x and x cannot be greater or less than itself. Or, to take another example, if x is the brother or sister of y, then y may not be the brother or sister of x, for y may be the same as x. The obvious reply would be that if x and y refer to the same person, x cannot be the brother or sister of y, since x cannot be the brother or sister of itself. But that is just what may be contended. One may argue that unless a relation is reflexive, it cannot be symmetrical, since in the exceptional event of y being interpreted to be the same as x, it will not hold good. Not merely this, even the statement of an irreflexive relation would become logically impossible, unless we introduce the limitation that y is not to be interpreted as x. Once, however, this limitation is accepted it would apply equally to transitivity and invalidate the argument given by Russell above.

It should be noted here that the reflexivity or irreflexivity of a relation is not so much a matter of logic as of intuitive judgment. Whether x can be greater than itself is not a matter of argument. Similarly, it may be contended that the transitivity of a relation is not a matter of argument, but of specific judgment. If x is greater than y, and y is greater than z, then the question of x being greater than z cannot be a subject of argument. For, if once the necessity of argument is admitted, it would have to be applied in the case of reflexivity also, where, perhaps, it cannot be applied at all.

The dilemma, by now, should be fairly obvious. Either we accept that a relation which is irreflexive can neither be sym-

metrical nor transitive, or we agree to the limitation on the interpretation of y or z and, thus, give up the position that a relation which is both symmetrical and transitive must be reflexive in character.

If we choose to deny transitivity to irreflexive relations, as we must if we do not accept the limitation on the interpretation of the symbols y, z, etc., we shall find it impossible to have an ordered series which can only be formed out of relations that are asymmetrical, transitive and connected. As asymmetry necessarily involves irreflexivity and as irreflexivity would, on the usual Russellian argument, necessarily involve intransitivity, the study of ordered series would become logically impossible.

Such an alternative will hardly be welcome to anybody. The acceptance of the limitation that a symbol is not to be interpreted the same as another symbol, unless allowed otherwise, would hardly disturb anything except the belief that symmetry and transitivity necessarily involve reflexivity.

It may be urged, however, that what we are asked to give up is not some idiosyncratic psychological belief but a strictly proved logical deduction. And as logicians we cannot give up a strictly deduced logical entailment even if it leads to empirical or non-empirical difficulties elsewhere.

But even though it is true there are strict proofs to show that reflexivity is entailed by symmetry and transitivity,[1] yet all these proofs assume the possible identity of the third and the first term of a transitive relation. If such an assumption be given up, the proof will not be possible and thus the incompatibility between the logical proof and the concrete example would have been removed. Otherwise, no relation which is irreflexive would be symmetrical or transitive and even the symbolical transcription of an asymmetrical relation would become a logical impossibility.

University of Sangar.

[1] A simple proof for such an entailment has already been given. A more complicated proof can be found in Quine's *Methods of Logic*, p. 179–80.

INSCRIPTIONALISM AND INDIRECT QUOTATION

By ISRAEL SCHEFFLER

SOME years ago, I proposed an inscriptional analysis of indirect quotation[1] and tried to show that it escaped the objections offered by Church[2] to Carnap's theory of belief-statements,[3] concluding that these objections therefore provide no conclusive argument against nominalistic interpretations of indirect discourse. Briefly put, my proposal was to construe the that-clauses, in indirect quoting contexts of the form ". . . . writes that——", as single indivisible predicates denoting concrete inscriptions forming rephrasals of one another; " Seneca writes that man is a rational animal " was thus to be taken as " There is an inscription *i*, such that *i* is both a that-man-is-a-rational-animal and written by Seneca ". My paper has since been discussed by Professor Church and others, and some general issues raised that call for further comment.

As remarked above, my examination of Church's arguments against Carnap's theory was intended to show that my inscriptional proposal could withstand their force, thus rebutting Church's suggestion that they might constitute " an insuperable objection " to " analyses that undertake to do away with propositions ". It is, therefore, somewhat misleading to say, as Professor Church now says, after adducing *new* arguments, " Scheffler has not established his claim to have provided a workable substitute for propositions ".[4] My claim was to have shown Church's original arguments ineffective in one sample inscriptional case, and hence no insuperable bar to a nominalistic interpretation. I had no illusions of having rebutted in advance all other anti-nominalistic arguments that might be constructed. Professor Church's new considerations are of independent interest and will be discussed below, but they do not show that my original claim has not been established.

Professor Hempel states,[5] indeed, that my argument in support of this claim " is quite convincing ", but doubts that the

[1] Scheffler, I., "An Inscriptional Approach to Indirect Quotation ", ANALYSIS, Vol. 14, No. 4 (1954), pp. 83–90.

[2] Church, A., " On Carnap's Analysis of Statements of Assertion and Belief ", ANALYSIS, Vol. 10, No. 5 (1950), pp. 97–99.

[3] Carnap, R., *Meaning and Necessity*, University of Chicago Press, 1947

[4] Church, A., " Propositions and Sentences ", in *The Problem of Universals* (A symposium by I. M. Bochenski, A. Church, and N. Goodman: The University of Notre Dame Press (Notre Dame, Indiana), 1956), p. 11.

[5] Hempel, C. G., " Review of Scheffler, 'An Inscriptional Approach to Indirect Quotation ' ", *Journal of Symbolic Logic*, Vol. 22, No. 1 (1957), p. 86.

" ultimate purpose of nominalistic analysis, namely, replacement of relatively unclear notions by clearer and more precise ones", is served by my " technically nominalistic " proposal. The that-clause predicates required by my proposal are, suggests Hempel, " hardly more intelligible than the notion of the proposition expressed by the corresponding that-clause ", for " the task of stating criteria of application for any particular that-clause predicate would seem to face the same difficulties as the task of specifying the propositional meaning of a particular sentence belonging to a given language and occurring in a definite context ".

This argument, however, seems to me to rest on a confusion of " clarity " in the sense of *philosophical intelligibility* with " clarity " in the sense of *determinacy of application*. Nominalistic philosophy, as I understand it, strives to increase the intelligibility of theories, not the determinacy or precision with which they can be applied. In showing how to avoid appeal to suspect entities such as entelechies, forces, properties, propositions, etc., nominalistic analyses are thought by nominalists *thereby* to increase intelligibility, though overall determinacy of application is unaffected. By meeting the requirements of technical nominalism with respect to eliminating commitment to propositions, my proposal is thus fully in accord with nominalistic goals. To suppose differently, on the basis of Professor Hempel's argument, we should have to rule that the interpretation of " is wise " in " Socrates is wise " as an indivisible, non-designative, predicate of individuals is no better from a nominalistic standpoint than a construal of the sentence as " Socrates is a member of the class Wise ", in view of the fact that to state criteria of application for the predicate is no easier than to specify rules of admission to the class. Analogously, no nominalist would need to balk at thinking of Good Fortune as an entity and describing Jones as having had this entity, since the alternative description of Jones as having been very fortunate is no more easily tied to explicit and general criteria of application.[2] In sum, to the nominalist eager to devise ways of avoiding theoretical commitment to pseudo-entities, Professor Hempel seems to say, " Why

[2] That a term is applied with great precision does not mean that it needs no explanation, but rather that it is deserving of explanation; e.g., it is because we *do* know how to apply numerical terms, counterfactual constructions, inductive notions, that explaining them becomes important. To hold that everyone who applies them proficiently understands them theoretically is clearly wrong. It follows that, for example, to render primitive causal or counterfactual notions explicit and precise through formalization is not to provide an explanation. Critics of logical methods in philosophy are, I think, (sometimes) rightly objecting to such formalizing of obscure notions as a substitute for explaining them.

go to all the trouble, when you can get along just as well *with* such commitments in applying your theory to cases? "[1]

If this question, far from representing the "ultimate purpose" of nominalism, sounds rather pragmatic in spirit, Professor Church's emphasis on the simplification effected by an abstract ontology sounds even more so. Arguing generally against finitistic nominalism in the sense of Goodman and Quine,[2] he suggests that a major difficulty is that "The theory is too complicated in application".[3] As is the case in mathematics and the natural sciences, he says, "a theory may be greatly simplified by incorporating into it additional entities beyond those which had originally to be dealt with, and I believe it to be a false economy which would forego simplification of a theory by such means. The notion of a concrete physical object, extended in space and persistent through time, is a case in point, as what had originally to be dealt with by the physical theories in which such objects appear was not these objects themselves but rather certain observations and physical experiences. Indeed the justification would seem to be basically the same for extended physical objects in macrophysical theory and for ideal sentences in logical syntax: both are postulated entities—some may prefer to say inferred entities—without which the theory would be intolerably complex if not impossible".[4]

This argument might, it seems to me, also be put forward in defence of such postulated and undeniably simplifying entities as the élan vital, the spirit of the age, the Will, the Devil, luck, the superego, the id, and the manifest destiny of nations. Surely, *without* these entities, the theory of man (besides losing a number of interesting characterizations and distinctions otherwise possible) becomes intolerably complex. Unfortunately, however, *with* these entities it seems to many people to become just intolerable. That is, it becomes unintelligible to them without further, complicating explanation. It is, for such people, simply beside the point that a theory they cannot understand has a surpassing simplicity as well as a demonstrated usefulness. It

[1] Professor Hempel objects also that to state conditions for the application of my term "rephrasal" is no easier than to do so for "synonymy". But my reason for using "rephrasal" is not that it is in any way clearer than "synonymy". Rather my argument is that synonymy is not adequate for the analysis of indirect discourse. See Scheffler, I., "On Synonymy and Indirect Discourse", *Philosophy of Science*, Vol. 22, No. I (1955), pp. 39–44, and especially footnote 11, and Hempel's review of this paper in *Journal of Symbolic Logic*, Vol. 22, No. 2 (1957), p. 208.

[2] The reference is to Goodman, N. and W. V. Quine, "Steps Toward a Constructive Nominalism", *Journal of Symbolic Logic*, Vol. 12, No. 4 (1947), pp. 105–122.

[3] "Propositions and Sentences", p. 11.

[4] *Ibid.*, p. 9.

is beside the point, that is to say, so far as intelligibility is in question, though possibly relevant from other standpoints, e.g., aesthetic elegance, or convenience in certain practical applications such as pedagogy, psychotherapy, propaganda, or prediction.

That intelligibility is not an automatic concomitant of theoretical simplicity and utility is amply indicated by the fact that Professor Church's chief example—the case of concrete physical objects justified by their simplifying of our physical knowledge—is also an example of a perennial philosophical problem: how to understand the existence of "external" physical reality. Philosophers who take this problem seriously are not simply bad physicists, nor are they therefore to be construed as Super-physicists, legislating to their scientific subordinates. Rather, they are concerned to render intelligible to themselves theories they gladly admit are useful, elegant, and of great importance. Their enterprise is no more to be judged by its scientific fertility than are scientific theories to be judged by their philosophical interest.

It follows that to insist on philosophical clarification of the ontology of a given theory is not generally to interfere with the normal scientific career of the theory. The language of "renunciation", "rejection", "giving up", and "doing without", in ontological discussions is thus dangerously ambiguous.[1] To state that a given scientific assumption is philosophically unintelligible is not to counsel giving it up in science, but rather to ask for further, philosophical explanation. When Professor Church, therefore, says, "I believe it to be a false economy which would forego simplification of a theory by such means", he apparently thinks of the nominalist as motivated by the desire to effect a scientific economy, or as somehow opposed to the development of platonistic theories as a scientific pursuit. On the contrary, as I see it, the nominalist's desire is to put into intelligible language (from his point of view) and so to *understand* what it is the scientist or the platonist is saying. To suppose that, because he tries to do this, he is somehow opposed to legitimate scientific simplication is like saying that, because I don't understand Sanskrit, I am somehow opposed to it.[2]

[1] See Goodman and Quine, *op. cit.*, first paragraph.

[2] Like explanatory discourses generally (say in the classroom), the nominalist's explanatory language is different from and, in one sense, more complicated than, the discourse explained. But no explanatory discourse is intended to supplant the original generally and there is another sense in which the explanatory discourse is simpler, i.e., it is more understandable. Thus an explanatory *definiens* may be both more complicated structurally and also more understandable than a given *definiendum*. The reader will by now have discerned some of the many different uses to which "simplicity" may be put, rendering obscure a variety of current philosophical arguments resting on some unspecified use, or confusing one with another.

Nor is anything so far said to be taken as conceding that the case of ideal sentences in logical syntax is indeed a scientific simplification analogous to simplifications in physical science. Thus far, I have argued that the philosopher's explanatory concern is independent of the scientist's or the practitioner's. But this does not imply that all postulations made in the name of science and simplicity are on a par. To revert to our earlier example, there are many who would distinguish sharply between the superego and the spirit of the age on purely scientific or pragmatic grounds: the latter, unlike the former, (it is said by these people) does no "causal" or "empirical" explaining, despite the fact that it helps to build neat historical theories. Like the dormitive virtue, it simplifies, but does no real scientific work. It is at least questionable whether ideal sentences are not more like the spirit of the age and the dormitive virtue than like the superego and the electron, since they are not tied to observation in any clear sense nor do they account for phenomena in the way acknowledged scientific theories do.[1]

Aside from considerations of relative simplicity, Professor Church now raises the question how to interpret a *new* group of sentences on a nominalistic basis:

(a) Church and Goodman have contradicted each other.
(b) Goodman will speak about individuals.
(c) Some assertions of Velikovsky are improbable.
(d) All assertions of Aristotle are falsehoods.

Church expresses doubt that either extensive analysis of the semantic predicates here or an axiomatic treatment of their relations to each other, to the that-clause predicates of my proposal, and to the syntax of inscriptions denoted by the latter, can be accomplished without reintroducing commitment to propositions. He does not offer a proof in support of his doubt, nor even a specific argument of the sort he offered previously against Carnap's analysis. In fact, he states, "The possibility remains that the claim [to have provided a workable substitute for propositions] might be substantiated by a longer and more detailed development, including solutions of the difficulties just discussed and treatment of a compatible finitistic syntax".[2] Professor Church, in effect, is pointing out that further problems remain for the nominalist. This is certainly true; as a matter of fact, some problems perhaps more naturally related to those

[1] For this argument I am indebted to N. Chomsky.
[2] "Propositions and Sentences", p. 11.

of indirect quotation remain. Meckler, for example, cites as a limitation of my paper its failure to suggest an analysis for belief-sentences.[1] The existence of further problems as yet unsolved can, however, hardly qualify as an argument against any philosophical programme. Much less can it serve to establish the validity of a rival programme, and in particular, one that faces much the same or analogous problems, e.g., the task, mentioned by Hempel, " of specifying the propositional meaning of a particular sentence belonging to a given language and occurring in a definite context "[2] and that of stating conditions under which two *inscriptions* express or name the same or contradictory propositions.

Finally, it is not a part of my proposal (as Professor Church appears to think it is) that accidentally occurring inscriptions may not be considered for any purpose.[3] I excluded consideration of them just in cases of explicit indirect quotation where, if the quoting statements are true, there exist appropriate non-accidentally occurring inscriptions. It does not follow that my peculiar cross-linguistic that-clause predicates must be used elsewhere, to analyse other statements than those of the form ". . . writes that——". It is conceivable that some problems could be handled by relating inscriptions generally through rephrasal-relationships to statements in an interpreted base language, characterized by a nominalistic syntax. With contradiction syntactically described for the base language, for example, Church's problem-sentence (a) might be interpreted as:

(a′) (∃x)(∃y)(∃z)(∃w)(Goodman inscribes x · Church inscribes y · Reph xz · Reph yw · Contradicts zw)

The same strategy seems feasible also for (b) and (c), the latter's analysis depending on characterization of a syntactic (or syntactic-pragmatic) relation *K* of confirmation in the base language as well as a description of accepted evidence-statements in this language. I am somewhat doubtful about this strategy in the case of (d), however, because of the general difficulty of defining truth and falsehood.[4] And still other problems, including that

[1] Meckler, L., "An Analysis of Belief-Sentences", *Philosophy and Phenomenological Research*, Vol. 16, No. 3 (1956), pp. 317–330, especially point (4) on p. 318.

[2] *Journal of Symbolic Logic*, Vol. 22, No. 1, (1957), p. 86.

[3] " Propositions and Sentences ", p. 10.

[4] A nominalized version of Tarski's definition for a sufficiently rich base language to incorporate rephrasals of all actual inscriptions (or those we care about) would seem to do the trick, however. This possibility is suggested by the paper of R. M. Martin and J. H. Woodger, " Toward an Inscriptional Semantics ", *Journal of Symbolic Logic*, Vol. 16, No. 3 (1951), pp. 191–203.

of belief-sentences, remain for the nominalist. No proof has been offered showing that they are insoluble. But even if they continue to resist the nominalist's best efforts, this will no more establish platonism than the continuing difficulties of physics establish the superiority of mysticism.

Harvard University.

MR. RESCHER ON RANDOM INDIVIDUALS

By L. GODDARD

IN his discussion of the logician's use of arbitrarily selected individuals ("Can there be Random Individuals?" ANALYSIS 18.5, pp. 114–117), Mr. Rescher claims to show that a contradiction follows if we accept the rule of inference,

$$\text{(II)}^1 \qquad \frac{\phi y}{\therefore (x)\phi x}$$

i.e. if 'ϕy' is a theorem then '$(x)\phi x$' is a theorem. The crucial step in his argument is that, using *modus tollens*, this rule implies another, viz.

$$\text{(III)} \qquad \frac{(\exists x)\sim\phi x}{\therefore \sim\phi y}$$

i.e. if '$(\exists x)\sim\phi x$' is a theorem then '$\sim\phi y$' is a theorem. I want to argue that this is not so.

That is, (II), unlike

$$\text{(I)} \qquad \frac{(x)\phi x}{\therefore \phi z}$$

(*i.e.* if '$(x)\phi x$' is a theorem then 'ϕz' is a theorem), cannot be represented as a law. It is a law of quantification logic that

(i) $(x)\phi x \rightarrow \phi z$ (*ie.* if $(x)\phi x$ then ϕz);

and indeed, given *modus ponens* as a rule of inference, (I) can be derived from (i). For since '$(x)\phi x \rightarrow \phi z$' is a theorem it follows that, if *modus ponens* is a rule, then,

if '$(x)\phi x$' is a theorem then 'ϕz' is a theorem.

But there is no law corresponding to (II), no law from which it can be derived. For manifestly,

[1] I follow Mr. Rescher in his numbering and notation (except for the small Roman numerals) but re-write each rule and law in terms of 'if . . . then . . .' to make the issues clearer.

(ii) $\phi y \rightarrow (x)\phi x$ (*i.e.* if ϕy then $(x)\phi x$)

is not a law of quantification logic.

Suppose, however, that (ii) is a law. Then by *modus tollens* we should have,

(iii) $(\exists x)\sim\phi x \rightarrow \sim\phi y$ (*i.e.* if $\sim(x)\phi x$ then $\sim\phi y$).

And just as (i) plus *modus ponens* entails (I), so this plus *modus ponens* entails (III). That is, if '$(\exists x)\sim\phi x \rightarrow \sim\phi y$' is a theorem then, by *modus ponens*,

if '$(\exists x)\sim\phi x$' is a theorem then '$\sim\phi y$' is a theorem.

But (II) does not have this consequence. Using *modus tollens* on (II), we obtain only,

(IIa) if '$(x)\phi x$' is not a theorem then 'ϕy' is not a theorem.

And this is not the rule (III)—which, for purposes of comparison, we may write as,

if '$\sim(x)\phi x$' is a theorem then '$\sim\phi y$' is a theorem.

One is a rule about "not-theorems", the other a rule about "theorem-nots"; and it is a property of quantification logic that these two concepts are not identical (disprovability is not the same as unprovability). Hence there are no general reasons which allow us to conclude that "'$(x)\phi x$' is not a theorem" means the same as "'$\sim(x)\phi x$' is a theorem" and that "'ϕy' is not a theorem" means the same as "'$\sim\phi y$' is a theorem" (from which it would follow that (IIa) and (III) are identical); on the contrary, there are general reasons against this.

The only question, therefore, is whether (IIa) (which we know to be derivable) implies (III). But this seems unlikely. Indeed if we take an arithmetical example, it is easily seen that there is a ϕ for which (IIa) holds and (III) fails. Thus, taking 'ϕ' as '$\ldots>10$', then, provided arithmetic is consistent, '$(x)(x>10)$' is not a theorem and neither is '$y>10$', for free 'y'. That is, (IIa) holds. (III) fails, however, since '$\sim(x)(x>10)$' is a theorem but '$\sim(y>10)$', with 'y' free, is not. If '$\sim(y>10)$' were a theorem, '$\sim(11>10)$' would be, and arithmetic would be inconsistent.

What it amounts to is that, in putting up (III) as a consequence of (II), Mr. Rescher has treated the rule (II) as if it were the law (ii). But (ii) is not a law, and (II) does not imply (III).

Unless (III) can be derived in some other way, therefore, the effect of this is to stifle the move from

$$(2) \quad (Ez)\sim(y=z)$$

to

$$(3) \quad \sim(y=y);$$

and hence to avoid the contradiction.

This is not, of course, to say anything about Mr. Rescher's non-formal arguments about the queerness of arbitrary selection as a logical technique. But here, too, there are difficulties. His phrase " random individuals " is extremely misleading. If it means, as it should, " arbitrarily selected individuals ", it is clear enough that what is intended is that the individual is *chosen at random*, *i.e.* without forethought. It is an actual member of the group from which it is chosen and, of course, once chosen, it is a specific individual with all the properties of individuals. Only in any arguments about it, its specific *distinguishing* characteristics, which enable us to recognise it as an individual different from the others in the group, are not taken into account. The only characteristics which are taken into account are those which it has in common with all other members of the group. Hence, the conclusion which is arrived at is independent of the individual chosen and therefore holds for all members of the group. This is why the rule (II) is accepted. But in using the phrase " random individual " Mr. Rescher seems to want to suggest that somehow or other the logician is intending to say that the individual has the property of randomness. " The ' arbitrariness ' or ' randomness ' resides not in the individuals ", he concludes. But whoever thought that it did? If the logician were saying this then of course he would be committing a category mistake. But he never does. There is the world of difference between a random (free) choice of actual individuals and the calculated choice of a random individual—whatever this might be.

University of New England, Armidale, N.S.W.

PARADOX LOST

By W. J. HUGGETT

MR. ROZEBOOM has presented a form of the Liar's Paradox which is apparently untouched by any appeal to "meaninglessness" or the like (ANALYSIS, 18.5, pp. 105–113). And since his argument is fully formalized and violates no logical principles, he concludes that "we are not going to find any cheap resolution of the Liar in terms of ambiguity" (p. 113). My purpose in what follows is to attempt one such resolution. I shall argue that his formulation of the Paradox requires revision and that once this revision is made no paradox remains to be conjured away.

The "more sophisticated" version of the Paradox he proposes is as follows:

(S)

In Figure 1, there is a sentence-token which conveys a false statement.

Figure 1

(Σ) In Figure 1, there is a sentence-token which conveys a false statement.

Given this paradigm and the linguistic habits of an English-speaking reader, a paradox may be readily shown to emerge. But before we get lost in paradox let us look a little more closely at the paradigm itself.

The source of the paradox is to be found in the fact that the *statement* designated by Σ and the *sentence-token* designated by S are represented by the same symbol sequence. Yet there are significant differences between Σ and S which an adequate symbolization should recognize and make evident. The crucial difference between the two is, of course, that Σ is asserted while S is not. However, assertion is suggested in both cases by the manner in which the verbs are put—erroneously suggested in the case of S. What is required, then, is a way of symbolizing S in which assertion is *not* suggested, and which will at the same time exhibit the structural similarity between Σ and S.

Following Hare,[1] we may regard a statement or assertion as consisting of two components: the phrastic, envisaging a possi-

[1] *The Language of Morals*, p. 18.

ble state of affairs, and the neustic, functioning as a sign of assertion. The general form of a statement will therefore be:

(a) Such-and-such's being the case, yes.
i.e.
(b) Such and such is the case.

and of a sentence-token will be:

Such-and-such's being the case.

This method of symbolizing statement and sentence-token brings out the essential point of difference between them (viz. that the sentence-token is not being asserted), a point of difference which ordinary language serves only to conceal.

Our problem now is to find a suitable phrastic for Σ and S and to reformulate the paradigm given above in the phrastic-neustic terminology. The following revised paradigm results:

(RS)

> In Figure 1, a sentence-token's conveying a statement, on being asserted, and its being asserted, and the asserted statement's being false.

Figure 1

(RΣ(a)) In Figure 1, a sentence-token's conveying a statement on being asserted, and its being asserted, and the asserted statement's being false, yes.

i.e.

(RΣ(b)) In Figure 1, there is a sentence-token which conveys a statement on being asserted, and it is being asserted, and the statement it asserts is false.

The revised form of Σ, RΣ, is a conjunction of three propositions A, B, and C. Proposition B states that RS is being asserted, and C states that what RS asserts is false. But our symbols for RS clearly reveal that RS is *not* being asserted. Hence B is false and, by the ordinarily accepted rules of logic, so is the conjunctive proposition RΣ. But from the falsity of RΣ we cannot infer the falsity of its last component C. And, indeed, since B is false no question of the truth value of C (which presupposes the truth of B) can conceivably arise. Neither can a truth value be assigned to RS. Since it is not being asserted, no statement has been made and there is no truth value. In the absence of assignable truth values for C and RS no paradox

emerges. Now we can see that the original sin committed in our first paradigm was that of prematurely assigning truth values.

Perhaps one further reflection will help us to place the present paradox in its proper perspective. The essential property of sentence-tokens is that they are spatially locatable and this fact is characteristically signified by placing them in an enclosed figure in the paradigm. If sentence-tokens did not have this property the Liar's Paradox could not be formulated. But as we have noted above, sentence-tokens are neither true nor false and as long as they maintain their status as sentence-tokens there is no paradox. On the other hand, once we *assert* a sentence-token we are dealing with statements and these are not locatable. (Unless we locate them in the knowing subject—wherever *that* may be!) We cannot speak of the statement as being in the enclosed figure, and so we cannot construct our paradox.

Whether or not genuine paradoxes of the Liar type can be constructed taking account of the considerations I have put forward here, I do not profess to know. My limited objective has been simply to show that what Rozeboom, Ushenko, and others have taken to be a paradox is not at all paradoxical.

University of Manitoba, Winnipeg.

LET EPIMENIDES LIE!

By C. H. WHITELEY

MR. W. W. ROZEBOOM, in a careful and ingenious treatment of the Liar type of paradox,[1] says "It seems most implausible that of a set of similarly situated sentence-tokens, all of which exemplify the same sentence-type, the mere physical locus of one could emasculate it of its power to convey the statement conveyed by its brothers". Plausible or not, this suggestion is true, and must be accepted if we are to understand how these paradoxes arise.

If Epimenides says "I am now lying", and nothing more, his remark suffers from two defects. Firstly, since he denies what in the same breath he also asserts, he contradicts himself. Secondly, he uses the word "lie", which, to be used intelligibly, presupposes that there is some sentence, other than the sentence in which it occurs, to which it refers (in Ryle's phraseology,[2] it

[1] ANALYSIS 18.5 (April 1958). [2] ANALYSIS 11.3 (January 1951).

" promises a namely-rider "); here, in the absence of any such statement, the lie Epimenides says he is telling cannot be specified. This latter defect would also belong to " I am now telling the truth ", " I am now giving you my opinion ", etc., if uttered alone.

If Epimenides says " Epimenides is now lying ", this is equivalent to " I am now lying ", uttered by Epimenides, and suffers from the same defects. But the same sentence can be uttered by anybody other than Epimenides without having either defect—it may, indeed, be true, and in any case gives rise to no paradoxes. If one wishes to frame the rules of the language in such a way that paradoxes cannot be produced, it is not sufficient to ban sentences like " I am now lying ", which is absurd whenever and by whomsoever it is uttered. One must also forbid *Epimenides* to say " Epimenides is now lying "; but one must not forbid anybody else to say it, since there may well be circumstances in which it very much needs to be said. Here, then, is one sort of sentence which makes good sense when uttered in some circumstances, but becomes absurd when uttered in different circumstances.

This is one sort of case in which the intelligibility of a sentence is affected by the circumstances in which it is uttered. Another is the way in which " We must wait till Christmas Day, 1958 ", makes good sense at any time up to and including December 24th, 1958, but ceases to do so on and after December 25th.

Paradoxes of the type Rozeboom presents depend on manoeuvres by which a sentence which ordinarily makes good sense is put into a context in which it cannot bear this sense. His sentence " In Figure 1, there is a sentence-token which conveys a false statement " is harmless so long as it stays outside Figure 1. Putting it inside Figure 1, and putting no other sentence in Figure 1, deprives it of any object of reference other than itself, and so makes it contradict itself. Thus the sentence inside the figure does not have the same meaning, or express the same proposition, as a similar sentence outside the figure; if it did, one would have to say that one and the same proposition can be both self-consistent and self-contradictory.

University of Birmingham